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II. *Of the Heat, &c. of Animals and Vegetables.*

By Mr. John Hunter, F. R. S.

Read June 19, and Nov. 13, 1777.

IN the course of a variety of experiments on animals and vegetables, I have frequently observed that the result of experiments in the one has explained the œconomy of the other, and pointed out some principle common to both; I have therefore collected some experiments which relate to the heat and cold of those substances. Having found variations in the degree of heat and cold in the same experiment, for which I could not account; I suspected that this might arise from some imperfection in the construction of the thermometer. I mentioned to Mr. RAMSDEN my objection to the common construction of that instrument, and my ideas of one more perfect in its nature, and better adapted to the experiments in which I was engaged. He accordingly made me some very small thermometers, six or seven inches long, not above $\frac{2}{12}$ ths of an inch thick in the stem; having the external diameter of the ball very little larger than that of
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the stem, on which was marked the freezing point. The stem was embraced by a small ivory scale so as to slide upon it easily, and retain any position. Upon the hollow surface of this scale were marked the degrees which were seen through the stem. By these means the size of the thermometer was very much reduced, and it could be applied to soft bodies with much more ease and certainty, and in many cases in which the former ones could not be conveniently applied: I therefore repeated with it such of my former experiments as were not originally satisfactory, and found the degrees of heat very different, not only from what I generally imagined, but also from what I had found in my former experiments with the thermometers of the common construction.

I have observed in a former paper^(a), and find it supported by every experiment I have made on the heat and cold of animals, that the more perfect have the greatest power of retaining a certain degree of heat, which may be called their standard heat, and allow of much less variation than the more imperfect animals: however, it will appear from the first, second, and third experiments, that many, if not all of them, are not capable of keeping constantly to one standard; but vary from their standard

(a) Vide Philosophical Transactions for the year 1775, vol. LXV. part II. p. 446.

heat,

heat, either by external applications, or disease. However, these variations are much greater below the standard heat than above it; the perfect animals having a greater power of resisting heat than cold, so that they are commonly near their ultimate heat. Indeed we do not want any other proof of this variation than our own feelings: we are all sensible of heat and of cold, which sensations could not be produced without an alteration really taking place in the parts affected; which alteration in the parts could not take place, if they did not become actually warmer or colder. I have often cooled my hands to such a degree, that I have put them into pump-water, immediately pumped, to warm them; therefore, my hands were really colder than the pump-water.

Real increase of heat must alter the texture or position of the parts, so as to produce the sensation called heat: and as this heat is diminished, the texture or position of the parts is altered in a contrary way; which, when carried to a certain degree, becomes the cause of the sensation of cold. Now these effects could not take place in either case without a real increase or decrease of heat in the part; heat, therefore, in its different degrees, must be present. When heat is applied to the skin, it becomes hot, in some degree, according to the application; and this may be carried so far as actually to burn the living

parts: on the contrary, in a cold atmosphere, a man's hand shall become so cold as to lose the sensation of cold altogether, and change it for that of pain. Real heat and cold can be carried so far, as even to alter the structure of the parts upon which the actions of life depend.

As animals are subject to variations in their degrees of heat and cold from external applications, they are of course, in this respect, affected in some measure like inanimate matter; and therefore, as parts are elongated or recede from the common mass, these effects more readily take place: for instance, all projecting parts and extremities, more especially toes, fingers, nose, ears, combs of fowls, particularly of the cock, are more readily cooled, and are therefore most subject to be affected by cold. Animals are not only subject to increase and decrease of heat, similar to inanimate matter; but the transition from the one to the other (as far as they allow themselves to go) is nearly as quick. However, I shall not confine myself to sensation alone, for it is in some degree ruled by habit: the habit of uniformity in the degree of the one or the other, will be the cause of a considerable increase of sensation from the smallest variation; while the habit of variation in the degree of heat and cold, will, in a proportional degree, prevent the sensation arising from either: but we shall be guided by actual experiment.

The parts above mentioned (*viz.* projecting parts and extremities) are such as will admit of the greatest change in their degrees of heat and cold, without materially affecting the animal. I find that they will raise or sink the thermometer, in some degree, according to the external heat or cold applied; although not in a proportional degree to this application, as would be the case in inanimate matter. Nor are the living parts cooled or heated in the same degrees, which appears from the application of the thermometer to the skin; for the cuticle may be considered as a dead covering, capable of taking greater degrees of heat or cold, than the living parts underneath can do; and it might be suspected, that the whole of the variation was in the covering. To remove this doubt I made the following experiments.

EXP. I. I sunk the ball of my thermometer under my tongue, which lay perfectly covered by all the surrounding parts, kept it there some minutes, and found that it rose to 97° ; having continued it some time longer there, I found it rose no higher. I then took several pieces of ice, about the size of walnuts, and put them in the same situation, allowing them to melt in part, but not wholly, that the application of cold might be better kept up, occasionally spitting out the water arising from the solution: this I continued for ten minutes, and found, on

introducing my thermometer, that it fell to 77° ; so that the mouth at this part had lost 20° of heat. It gradually rose to 97° again; but the thermometer in this experiment did not sink so low as it would have done in the hand, if a piece of ice had been held in it so long. Perhaps one reason may be assigned: the surface under the tongue being surrounded with warm parts, renders it next to an impossibility to cool it to any greater degree: but I suspect still another reason, *viz.* parts which have been in a habit of considerably varying in this respect, as the hand, will allow of greater latitude, being as it were insensibly drawn into cold, nor so susceptible of it, as has been already observed.

As a further proof, that the more perfect animals are capable of varying their heat, in some degree, according to the external heat applied, I shall adduce the following experiments made on the human subject.

The mouth being a part so frequently in contact with the external atmosphere in the action of breathing, whatever is put into it will be supposed to be influenced by that atmosphere; this will always render an experiment made in the mouth, relative to heat and cold, in some degree doubtful. I imagined that the urethra would answer better, because it is an internal cavity, and can be only influenced by heat and cold applied to the
external

external skin of the parts. I imagined also, that whatever effects heat or cold might have, when applied, would sooner take place in the urethra than in any other part of the body, as it is a projecting part; and therefore, if living animal matter was in any degree subject to the common laws of matter in this respect, the urethra would be readily affected: for this purpose I got a person, who allowed me to make such experiments as I thought necessary.

EXP. II. I introduced the ball of my thermometer into the urethra about an inch; after it had remained there a minute, the quicksilver rose only to 92° ; at two inches, it rose to 93° ; at four inches, the quicksilver rose to 94° ; and when the ball had got as far as the bulb of the urethra, where it is surrounded by warm parts, the quicksilver rose to 97° .

EXP. III. These parts being immersed in water heated only to 65° for one minute, and the thermometer introduced about an inch and a half into the urethra, the quicksilver rose to 79° : this was repeated several times with the same success. To find if there was any difference in the quickness of the transition of heat and cold in living and dead parts, and also if the latitudes to which each would go were also different, I made the following experiments.. As this (*viz.* the urethra) still
appeared

appeared to me to be the very best part of any animal body for experiments of this kind, I had recourse to it; and as all comparative experiments should be as similar to one another as possible, excepting in those points where the difference (if there is any) makes the essential part of the experiment, I procured a dead penis.

EXP. IV. The heat of the penis of a living person, an inch and a half in the urethra, was 92° exactly. I first heated the dead one to the same degree; and then had the living one immersed in water at 50° , at the same time immersing the dead one in the same water; when, introducing the thermometer at different times, I observed their comparative quickness in cooling from 92° . The dead one cooled faster; but only by two or three degrees. The living came down to 58° , and the dead to 55° . After having continued the thermometer there some time longer, it fell no lower. I repeated the same experiment several times, with the same success; although sometimes there was a small difference in the degrees of heat from those of others, the heat of the water also differing; but the difference in the result was nearly in proportion, in all the three different trials, therefore the same conclusions are to be drawn from them. In these last experiments we find very little difference between the cooling of a part of a dead body, and that of the living; but we cannot suppose that this can take place

place through the whole body, as in this case a living man should always be of the same degree of heat with the atmosphere in which he lives. The man not choosing to be cooled lower than 53° or 54° , put it out of my power to see if the powers of generating heat were exerted in a higher degree, when the heat was brought so low as to threaten destruction; but from some experiments on mice, which will be related hereafter, it will appear, that the animal powers are called upon to exert themselves in this, when necessary.

From the experiments related I found, that parts of an animal were capable of becoming much colder than the common or natural heat: I therefore made farther experiments, with a view to see whether the same parts were capable of becoming much hotter than the standard heat of animals. The experiments were made in the same manner as the former, only the water was now hotter than the natural heat of the animal.

EXP. V. The natural heat of the parts being 92° , they were now immersed in water heated to 113° for two minutes, and the thermometer being introduced as before, the quicksilver rose to $100^{\circ}\frac{1}{2}$. This experiment I also repeated several times, but could not raise the heat of the penis beyond $100^{\circ}\frac{1}{2}$: this was probably owing to the person not being able at this time to bear the application of water warmer than 113° . As these were only single
expe-

experiments, I chose to make a comparative one with the dead part.

EXP. VI. Both the living and dead part being immersed in water, gradually made warmer and warmer from 100° to 118° , and continued in this heat for some minutes, the dead part raised the thermometer to 114° , while the living could not raise it higher than $102^{\circ}\frac{1}{4}$. It was observed, by the person on whom the experiment was made, that, after the parts had been in the water about a minute, the water did not feel hot; but, on its being agitated, it felt so hot that he could hardly bear it. Upon applying the thermometer to the sides of the living gland, the quicksilver immediately fell from 118° to about 104° , while it did not fall above a degree when put close to the dead; so that the living gland produced a cold space of water around it^(b).

EXP. VII. The heat of the rectum in the same man was $98^{\circ}\frac{1}{2}$ exactly.

In the second, third, fourth, fifth, and sixth experiments, we had an internal cavity, which is both very vascular and sensible, evidently influenced by external heat and cold, though only applied to the skin of the part;

(b) This might furnish an useful hint respecting bathing in water, whether colder or warmer than the heat of the body: for if intended to be either colder or hotter, it will soon be of the same temperature with that of the body; therefore in a large bath, the patient should move from place to place: and in a small one, there should be a constant succession of water of the intended heat.

while,

while, in the seventh experiment, another part of the same body, where external heat and cold can make little or no impression, was of the standard heat. Although we shall find hereafter, from experiment, that the rectum is not the warmest part of an animal; yet, in order to determine how far the heat could be increased by stimulating the constitution to a degree sufficient to quicken the pulse, I repeated the seventh experiment after the man had eaten a hearty supper, and drank a bottle of wine, which increased the pulse from 73° to 87° , and yet the thermometer only rose to $98^{\circ}\frac{1}{2}$.

Having formerly made experiments upon dormice in the sleeping season, with a view to see if there was any alteration in the animal oeconomy at that time, I find amongst these experiments the following which appear to be to our present purpose: but, that I might be more certain of the accuracy of my former experiments, I repeated them with my new thermometer.

EXP. VIII. In a room, in which the air was at between 50° and 60° of temperature, a small opening was made in the belly of a dormouse, of a sufficient size to admit the ball of my thermometer, which, being introduced into the belly at about the middle of that cavity, rose to 80° , and no higher.

EXP. IX. The mouse was put into a cold atmosphere of 15° above 0, and left there for fifteen minutes; after which, the thermometer being introduced a second time, it rose to 85° .

EXP. X. The mouse was again put into a cold atmosphere for fifteen minutes more; and the thermometer being then introduced, the quicksilver rose to 72° only, but gradually came up to 83° , 84° , and 85° .

EXP. XI. It was put a third time into the cold atmosphere, and allowed to stay there for thirty minutes; the lower part of the mouse was at the bottom of the dish, and almost frozen; the whole of the animal was a little numbed, and a good deal weakened. When the thermometer was introduced, it varied according to the different parts of the belly; in the pelvis, near the parts most exposed to the cold, it was as low as 62° ; in the middle, among the intestines, about 70° ; but near the diaphragm it rose to 80° , 82° , 84° , and 85° ; so that in the middle of the body the heat had decreased 10° . Finding a variation in different parts of the same cavity in the same animal, I repeated the same experiments upon another dormouse.

EXP. XII. I took a healthy dormouse, which had been asleep in a room in which there was a fire (the atmosphere at 64°); I put the thermometer into its belly, nearly at the
middle,

middle, between the thorax and pubis, and the quicksilver rose to 74° or 75° ; when I turned the ball towards the diaphragm, it rose to 80° ; and when I applied it to the liver, it rose to $81^{\circ}\frac{1}{2}$.

EXP. XIII. The mouse was put into an atmosphere at 20° , and left there half an hour; when taken out, it was very lively, much more so than when put in. I introduced the thermometer into the lower part of the belly, and it rose to 91° ; and upon turning it up to the liver, to 93° .

EXP. XIV. The animal was put back into the cold atmosphere at 30° for an hour, when the thermometer was again introduced into the belly; at the liver it rose to 93° ; in the pelvis, to 92° : it was still very lively.

EXP. XV. It was again put back into the cold atmosphere at 19° , and left there an hour; the thermometer at the diaphragm was 87° ; in the pelvis, 83° ; but the animal was now less lively.

EXP. XVI. It was put into its cage, and two hours after the thermometer, placed at the diaphragm, was at 93° .

From these experiments we have actual heat increased and decreased by the application of external cold; and likewise the heat varied according to the powers of life, as well in the same parts, as also in the different parts, of the same animal; for at first the natural heat of the

animal was much below the common standard, and, by the application of cold, and the powers of resistance to the cold being thus increased, the heat was considerably augmented; but when the animal was weakened by those exertions, it fell off with respect to the power of producing heat, and this in proportion to the distance from the heart.

Why the heat of this animal should be so low as 80° in an atmosphere of between 50° and 60° , is not easily accounted for, except upon the principle of sleep. But I should very much suspect, that the simple principle of sleep is out of the question, as sleep is an effect that takes place in all degrees of heat and cold. In those animals where the voluntary actions are suspended, it appears to be an effect arising from a certain degree of cold acting as a sedative, under which the animal faculties are proportionably weakened, but still retain the power of carrying on all the functions of life under such circumstances; but beyond this degree cold seems to act as a stimulant, and the animal powers are roused to action for self-preservation. It is more than probable, that most animals are under this predicament; and that every order has its degree of cold, in which the voluntary actions can be suspended.

When man is asleep, he is colder than when awake; and I find, in general, that the difference is about one de-

gree and a half, sometimes less. But this difference in the degree of cold between sleeping and waking is not a cause of sleep, but an effect; for many diseases produce a much greater degree of cold in the animal, without giving the least tendency to sleep; therefore the inactivity of animals from cold is different from sleep. Besides, all the operations of perfect life are going on in the time of natural sleep, at least in the perfect animals, such as digestion, sensations, &c.; but none of these operations are performed in the latter tribe.

To see how far the result of these experiments upon dormice was peculiar to them, I repeated the same experiments upon common mice. I procured two; one strong and vigorous, the other weakened by fasting.

EXP. XVII. The common atmosphere being at 60° , I introduced the thermometer into the abdomen of the strong mouse: the ball being at the diaphragm, the quicksilver was raised to 99° , but at the pelvis only to $96\frac{3}{4}$.

Here there was a real difference of about 9° in two animals of the same size, in some degree of the same genus, and at the same season of the year, and the atmosphere of nearly the same temperature.

EXP. XVIII. The same mouse was put into a cold atmosphere of 13° , for an hour, and then the thermometer

was

was introduced as before; the quicksilver at the diaphragm was raised to 83° , in the pelvis only to 78° .

Here the real heat of the animal was diminished 16° at the diaphragm, and 18° in the pelvis.

EXP. XIX. In order to determine whether an animal that is weakened, has the same powers, with respect to preserving heat and cold, as one that is vigorous and strong, I introduced the ball of the thermometer into the belly of the weak mouse; the ball being at the diaphragm, the quicksilver rose to 97° ; in the pelvis to 95° : the mouse being put into the cold atmosphere as the other, and the thermometer again introduced, the quicksilver stood at 79° at the diaphragm, and at 74° in the pelvis.

In this experiment the heat at the diaphragm was diminished 18° , in the pelvis 21° .

Here was a diminution of heat in the second greater than in the first, we may suppose proportional to the decreased power of the animal arising from want of food.

To determine how far different parts of other animals than those mentioned were of different degrees of heat; I made the following experiments upon a healthy dog.

EXP. XX. The ball of the thermometer was introduced two inches within the rectum, the quicksilver rose to $100^{\circ}\frac{1}{2}$ exactly. The chest of the dog was opened, and
a wound

a wound made into the right ventricle of the heart, and the ball immediately introduced; the quicksilver rose to 101° exactly. A wound was next made some way into the substance of the liver; and the ball being introduced, the quicksilver rose to $100^{\circ}\frac{3}{4}$. It was next introduced into the cavity of the stomach, where it stood exactly at 101° . All these experiments were made in a few minutes.

EXP. XXI. The same experiments were made upon oxen; the quicksilver rose exactly to $99^{\circ}\frac{1}{2}$.

EXP. XXII. The same were also made upon a rabbit, and the quicksilver rose to $99^{\circ}\frac{1}{2}$.

From the experiments on mice, and those upon the dog, it plainly appears, that every part of an animal is not of the same degree of heat; and hence we may reasonably infer, that the heat of the vital parts of man is greater than what it is found to be either in the mouth, the rectum, or the urethra.

To determine how far my idea, that animals could have their heat varied in proportion to their imperfections, is just, I made the following experiments upon fowls, which I consider to be one remove below what are commonly called quadrupeds.

EXP. XXIII. I introduced the ball of the thermometer successively into the *intestinum rectum* of several hens,
and

and found that the quicksilver rose as high as 103° , $103^{\circ}\frac{1}{2}$, and in one of them to 104° .

EXP. XXIV. I made the same experiments on several cocks, and the result was the same.

EXP. XXV. To determine if the heat of the hen was increased when she was prepared for incubation, I repeated the twenty-third experiment upon several sitting or clocking hens; in one the quicksilver rose to 104° ; in the others, to $103^{\circ}\frac{1}{2}$, 103° , as in the twenty-third experiment.

EXP. XXVI. Under the hen, who raised the quicksilver to 104° , I placed the ball of the thermometer, and found the heat there as great as in the rectum.

EXP. XXVII. I took some of the eggs from under the same hen, where the chick was about three parts formed, broke a hole in the shell, &c. and introduced the ball of the thermometer, and found that the quicksilver rose to $99^{\circ}\frac{1}{2}$. In some that were addled, I found their heat not so high by two degrees; so that the life in the living egg assisted in some degree to support its own heat.

It may be asked, whether those three or four degrees of heat, which are found in the fowl more than in the quadruped, are for the purpose of incubation? We found that the heat of the eggs, which was caused and supported by this heat, was not above the standard of the quadrupeds; and

and that it must probably have been less, if the heat of the hen had not been so great.

Finding from the above experiments, that fowls were some degrees warmer than that class commonly called quadrupeds (although certainly not so perfect animals) I chose to continue the experiments upon the same principles, and made the following upon those of a still inferior order. The next remove from the fowl are those commonly called amphibious.

EXP. XXVIII. I took a healthy viper, and introduced the thermometer into its stomach and anus; the quicksilver rose from 58° (the heat of the atmosphere in which it was) to 68° ; so that in a common atmosphere it is 10° warmer.

EXP. XXIX. The viper was put into a pan, and the pan into a cold mixture of about 10° ; after being there about ten minutes, its heat was reduced to 37° . It was allowed to stay ten minutes longer, the mixture being at 13° , and its heat was reduced to 35° . It was allowed to stay ten minutes more, the mixture at 20° , its heat at 31° , and it did not become lower; its tail was beginning to freeze; and it was now very weak. It may be remarked, that it became cold much slower than many of the following animals.

The frog being, in its structure, more similar to the viper than to either fowl or fish, I made the following experiments on that animal.

EXP. XXX. I introduced the ball of the thermometer into its stomach, and the quicksilver stood at 44° . I then put it into a cold mixture, and the quicksilver sunk to 31° ; the animal appeared almost dead, but recovered very soon: beyond this point it was not possible to lessen the heat, without destroying the animal. But its decrease of heat was quicker than in the viper, although the mixture was nearly the same.

The next order of animals were fish.

EXP. XXXI. I ascertained the heat of water in a pond, where there were carp, and found it $65^{\circ}\frac{1}{2}$. I then took a carp out of the same water, and introduced the thermometer into the stomach; the quicksilver rose to $69^{\circ}\frac{1}{2}$; so that the difference between the water and the fish was only $3^{\circ}\frac{1}{2}$.

EXP. XXXII. In an eel, the heat in the stomach, which at first was at 37° , sunk, after it had been some time in the cold mixture, to 31° . The animal at that time appeared dead, but was alive the next day.

EXP. XXXIII. In a snail, whose heat was at 44° , it sunk, after it had been put into the cold mixture, to 31° , and then the animal froze.

EXP. XXXIV. Several leaches having been put into a bottle, and that bottle immerfed in the cold mixture, the ball of the thermometer being placed in the middle of them, the quickfilver funk to 31° ; and by continuing the immerfion for a fufficient time to deftroy life, the quickfilver rofe to 32° , and then the leaches froze. In all thefe experiments none of the animals returned to life when they became thawed.

Finding that thefe imperfect claffes of animals are capable of varying their heat to that ftandard which can freeze the folids or fluids when dead, and not much farther before death enfues, I wifhed to determine to what degree of heat the animal could be brought.

EXP. XXXV. A healthy viper was put into an atmofphere of 108° , and allowed to ftay feven minutes, when the heat of the animal in the ftomach and anus was found to be $92^{\circ}\frac{1}{2}$, beyond which it would not rife in the above heat. The fame experiment was made upon frogs with nearly the fame fuccels.

EXP. XXXVI. An eel very weak, its heat at 44° , which was nearly that of the atmofphere, was put into water at 65° , for fifteen minutes; and, upon examination, it was of the fame degree of heat with the water.

EXP. XXXVII. A tench, whole heat was 41° , was put into water at 65° , and left there ten minutes; the

ball of the thermometer being introduced both into the stomach and rectum, the quicksilver rose to 55° . These experiments were repeated with nearly the same success.

To determine whether life had any power of resisting heat and cold in these classes of animals, I made comparative trials between living and dead ones.

EXP. XXXVIII. I took a living and a dead tench, and a living and a dead eel, and put them into warm water; they all received heat equally fast; and when they were put into the cold, both the living and the dead received it equally.

I long suspected, that the principle of life was not wholly confined to animals, or animal substance endowed with visible organization and spontaneous motion; but I conceived, that the same principle existed in animal substances, devoid of apparent organization and motion, where the power of preservation simply was required.

I was led to this notion twenty years ago, when I was making drawings of the growth of the chick in the process of incubation. I then observed, that whenever an egg was hatched, the yolk (which is not diminished in the time of incubation) was always perfectly sweet to the very last; and that part of the albumen, which is not expended on the growth of the animal, some days before hatching,

was also perfectly sweet; although both were kept in a heat of 103° , in the hen's egg for three weeks, and in the duck's for four; but I observed, that if an egg was not hatched, that egg became putrid in nearly the same time with any other dead animal matter.

To determine how far eggs would stand other tests of a living principle, I made the following experiments.

EXP. XXXIX. I put an egg into cold at about 0, and froze it, then allowed it to thaw; from this process I conceived, that the preserving powers of the egg must be lost. I then put this egg into the cold mixture, and with it one newly laid; and the difference in freezing was seven minutes and a half, the fresh one taking so much longer time in freezing.

EXP. XL. A new laid egg was put into a cold atmosphere, fluctuating between 17° and 15° ; it took above half an hour to freeze; but, when thawed and put into an atmosphere at 25° , it froze in half the time. This experiment was repeated several times, with nearly the same success.

To determine the comparative heat between a living and a dead egg, and also to determine whether a living egg be subject to the same laws with the more imperfect animals, I made the following experiments.

EXP.

EXP. XLI. A fresh egg, and one which had been frozen and thawed, were put into the cold mixture at 15° ; the thawed one soon came to 32° , and began to swell and congeal; the fresh one sunk to $29^{\circ}\frac{1}{2}$, and in twenty-five minutes after the dead one, it rose to 32° , and began to swell and freeze.

The result of this experiment upon the fresh egg was similar to the above experiments upon the frog, eel, snail, &c. where life allowed the heat to be diminished 2° or 3° below the freezing point, and then resisted all further decrease; but the powers of life were expended by this exertion, and then the parts froze like any other dead animal matter.

From these experiments in general it must appear, that a fresh egg has the power of resisting heat, cold, and putrefaction, equal to many of the more imperfect animals; and it is more than possible, that this power arises from the same principle in both.

From some of these experiments it appears, that the more imperfect animals are capable of having their heat and cold varied very considerably, not according to the extent of the heat or cold of the surrounding medium in which they can live, but according to the degree of cold which is capable of altering the parts in a dead state, below which the living power will not go far;
for

for whenever the surrounding cold brings them to that degree, the power of generating heat takes place till life is gone, then the animal freezes, and is immediately capable of admitting any degree of cold.

From these circumstances of those imperfect animals (upon which I made my experiments) varying their heat so readily, we may conclude, that heat is not so very essential to life in them as in the more perfect; although it be essential to many of the operations, or what may be called the secondary actions of life, such as digesting food^(b), and the propagation of their species, which requires the greatest power an animal can exert, more especially the last; and, as most of the more perfect of these imperfect animals are commonly employed in the first, we may suppose their heat to be such as this action of life requires, although in them it be never essentially necessary to be so high as to produce propagation^(c).

Therefore

(b) How far this idea holds good with fish I am not certain.

(c) How far the animal heat is lowered in the more perfect animals, when these secondary actions are not necessary, as in the bat, hedge-hog, bear, &c. I have not been able to determine, not having opportunities of examining these animals in their involuntary state. Dornice are in a mixed state between the voluntary and involuntary, and we find the heat diminished when the actions are not vigorous; and from a general review of this whole subject it would appear, that a certain degree of heat in the animal is necessary for digestion, and that necessary heat will be according to the nature of the animal. A frog will digest food when its heat is at 60°, but not when at 35° or 40°; and it is

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Therefore, whenever these imperfect animals are in a cold so low as to weaken their powers, and disable them from performing the first of these secondary actions, they become in some measure involuntary, and remain in a torpid state during the degree of cold which will always occur in some part of the winter in such countries as they inhabit; and the food of such animals in general not being produced in the cold season, affords another reason for their torpidity.

From the circumstances of their heat being allowed to sink to the freezing point, or somewhat lower, and then becoming stationary; and of the animal not being able to support life in a much greater degree of cold for a considerable time, we see a reason why those animals always endeavour to procure such places of abode in the winter as seldom arrive at that point. Thus we have toads burrowing, frogs living under large stones, snails protected under the shelter of stones and in holes, fish hav-

very probable that, when the heat of the bear, hedge-hog, dormouse, bat, &c. is reduced to 70° , 75° , or 80° , they lose their power of digestion; or rather that the body, in such a degree of cold, has no call upon the stomach. That animals, in a certain degree of heat, must always have food, is further illustrated by the instance of bees. The construction of a bee is very similar to a fly, a wasp, &c. A fly and a wasp can allow their heat to diminish as in the fish, snake, &c. without losing life, but a bee cannot; therefore a bee is obliged to keep up its heat as high as what we may call its digestive heat, but not its propagating; for which purpose they provide against such cold as would deprive them even of their digestive heat, if they had not food to preserve it.

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ing recourse to deep water, all which places are generally above the freezing point in our hardest frosts: however, our frosts are sometimes so severe as to kill many whose habitations are not very secure.

When the frost is more intense and of longer standing than common, or in countries where the winters are always severe, there is generally snow, and the water freezes: the advantage arising from these two circumstances are great; the snow serving as a blanket to the earth, and the ice to the water^(e).

(e) Snow and ice are perhaps the worst conductors of heat of any substance yet known. In the first place, they never allow their own heat to rise above the freezing point, so that no heat can pass through ice or snow when at 32° , by which means they become an absolute barrier to all heat that is at or above that degree; so that the heat of the earth, or whatever substance they cover, is retained: but they are conductors of heat below 32° . Perhaps that power decreases in proportion as the heat decreases under that part.

In the winter 1776 a frost came on, the surface of the ground was frozen; but a considerable fall of snow also came on, and continued several weeks; the atmosphere at this time was often at 15° , but it was not allowed to affect the surface of the earth considerably, so that the surface of the ground thawed, and the earth retained the heat of 34° , in which beans and peas grow.

The same thing took place in water, in a pond where the water was frozen on the surface to a considerable thickness; a large quantity of snow fell and covered the ice; the heat of the water was preserved and thawed the ice, and the snow at its under surface was found mixed with the water.

The heat of the water under the snow was at 35° , in which the fish lived very well.

It would be worthy of the attention of the philosopher, to investigate the cause of the heat of the earth, upon what principle it is preserved, &c.

As all the experiments I ever made upon the freezing of animals, with a view to see if it were possible to restore the actions of life when thawed, were made upon whole ones, and as I never saw life return by thawing^(f); I wished to see how far parts were similar to the whole in this respect; especially as we have it asserted, and with some authority, that parts of a man may be frozen, and afterwards recover: for this purpose I made the following experiments upon an animal of the same order as ourselves.

In January 1777, I mixed salt and ice till the cold was about 0; on the side of the vessel was a hole, through which I introduced the ear of a rabbit. To carry off the heat as fast as possible, it was held between two flat pieces of iron that went farther into the mixture. That part of the ear projecting into the vessel became stiff, and when cut did not bleed; and the part cut off by a pair of scissors, flew from between the blades like a hard chip.

The ear remained in the mixture nearly an hour: when taken out it soon thawed, and began to bleed; it became very flaccid, so as to double upon itself, having lost its natural elasticity. When out of the mixture nearly an hour, it became warm, and this warmth in-

(f) Vide Phil. Transf. for the year 1775, vol. LXV. part. II. p. 446.

creased to a considerable degree; while the other ear continued in its usual cold, and also began to thicken. The day following the frozen ear was still warm; and two days after it still retained its heat and thickness, which continued for many days after.

About a week after this, the mixture being the same as the former, I introduced both ears of the same rabbit through the hole, and froze them both: the found one, however, froze first, probably from its being considerably colder at the beginning. When withdrawn, they soon thawed, and soon both became warm, and the fresh ear thickened as the other had done before.

Feb. 23, 1777, I repeated these experiments. I froze the ear of a white rabbit till it became as hard as a board. It was longer in thawing than in the former experiment, and much longer before it became warm; however, in about two hours it became a little warm, and the day following it was very warm and thickened.

In the spring 1776, I observed that the cocks I had in the country had their combs smooth with an even edge, and not so broad as formerly, appearing as if near one half of them had been cut off. Having inquired into the cause of this, my servant told me, that it had been common in that winter during the hard frost. He observed, that they had become in part dead, and at last dropped off:

also, that the comb of another cock had dropped intirely off, which I did not see, as by accident he burnt himself to death. I naturally imputed this effect to those combs having been frozen in the time of the severe frost; and having, consequently, lost the life of that part by this operation. I endeavoured to try the solidity of this reasoning by experiment.

I attempted to freeze the comb of a very large young cock (which was of a considerable breadth) but could only freeze the serrated edges (which processes were full half an inch long); the comb itself being very thick and warm resisted the cold. The frozen parts became white and hard; and, when I cut off a little bit, it did not bleed, nor did the animal shew any signs of pain. I next introduced into the cold mixture one of his wattles, which was very broad and thin; it froze very readily: upon thawing both the comb and wattle, they became warm, but were of a purple colour, having lost that transparency which the other parts of the comb and the other wattle had. The wound in the comb now bled freely.

Both comb and wattle recovered perfectly in about a month. The natural colour returned first nearest to the sound parts, increasing gradually till the whole was become perfectly sound.

There was a very material difference in the effect between those fowls, the ferrated edges of whose combs I suspected to have been frozen in the winter of 176 $\frac{5}{6}$, for they must have dropped off. The only way in which I can account for this difference is, that in those fowls the parts were kept so long frozen, that the unfrozen or active parts had time to inflame, and had brought about a separation of the frozen parts, treating them exactly as dead, similar to a mortified part; and that before they thawed, the separation was so far completed as to deprive them of farther support.

As it is confidently asserted, that fish are often frozen and come to life again, and as I had never succeeded in any of my experiments of this kind upon whole fish; I made some partial experiments upon this class of animals, being led to it by having found a material difference in my experiments upon whole individuals and only parts of the more perfect order of animals.

I froze the tail of a tench (as high as the anus) which became as hard as a board; when it thawed, that part was whiter than common; and when it moved, the whole tail moved as one piece, and the termination of the frozen part appeared like the joint on which it moved.

On the same day I froze the tails of two gold fish till they became as solid as a piece of wood. They were put
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into cold water to thaw: they appeared at first, for some days, to be very well; but that part of the tail which had been frozen had not the natural colour, and the fin of the tail became ragged. About three weeks after a furr came all over the frozen part; the tail became lighter, so that the fish was suspended in the water perpendicularly, and they had almost lost the power of motion; at last they died. The water in which they were kept was New River water, shifted every day, and about ten gallons in quantity.

I made similar experiments upon an order of animals still inferior, *viz.* common earth worms.

I first froze the whole of an earth worm as a standard; when thawed it was perfectly dead.

I then froze the anterior half of another earth worm; but the whole died.

I next froze the posterior half of an earth worm; the anterior half lived, and separated itself from the dead part.

As I had formerly in making my experiments upon animals, relative to heat and cold, made similar ones on vegetables, and had generally found a great similarity between them in these respects, I was led to pursue the subject upon the same plan; but I was still farther induced to continue my experiments upon vegetables, as

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I imagined I saw a material difference between them in their power of supporting cold.

From observations and the foregoing experiments it plainly appears, that the living principle will not allow the heat of such animals to sink much lower than the freezing point, although the surrounding atmosphere be much colder, and that in such a state they cannot support life long; but it may be observed, that most vegetables of every country can sustain the cold of their climate. In very cold regions, as in the more Northern parts of America, where the thermometer is often 50° below 0, where peoples feet are known to freeze and their noses to drop off if great care be not taken, yet the spruce-fir, birch, juniper, &c. are not affected.

Yet that vegetables can be affected by cold, daily experience evinces; for the vegetables of every country are affected if the season be more than ordinarily cold for that country, and some more than others; for in the cold climates abovementioned, the life of the vegetable is often obliged to give way to the cold of the country: a tree shall die by the cold, then freeze and split into a great number of pieces, and in so doing produce considerable noise, giving loud cracks which are often heard at a great distance.

In this country the same thing sometimes happens to exotics from warmer climates: a remarkable instance of
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this kind happened this winter in his Majesty's garden at Kew. The *Erica arborea* or Tree-heath, a native of Spain and Portugal, which had kept its health extremely well against a garden-wall for four or five years, though covered with a mat, was killed by the cold, and then being frozen split into innumerable pieces^(g). But the question is, is every tree dead that is frozen? I can only say, that in all the experiments I ever made upon trees and shrubs, whether in the growing or active state, or in the passive, that whole or part which was frozen, was dead when thawed.

The winter 177 $\frac{5}{6}$ afforded a very favourable opportunity for making experiments relative to cold, which I carefully availed myself of. However, previous to that winter, I had made many experiments upon vegetables respecting their temperature comparatively with that of the atmosphere, and when they were in their different states of activity: I therefore examined them in different seasons, with a

(g) This must be owing to the sap in the tree freezing, and occupying a larger space when frozen than in a fluid state, similar to water; and that there is a sufficient quantity of sap in a tree newly killed is proved by the vast quantity which flows out upon wounding a tree. But what appeared most remarkable to me was, that in a walnut-tree, on which I made many of my experiments, I observed that more sap issued out in the winter than in the summer. In the summer, a hole being bored, scarcely any came out; but in the winter it flowed out abundantly.

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view to see what power vegetables have. I shall relate these experiments in the order in which they were made.

They were begun in the spring, the actions of life upon which growth depends being then upon the increase; and they were continued till those actions were upon the decline, and also when all actions were at an end, but whilst the passive powers of life were still retained.

The first were made on a walnut tree, nine feet high in the stem, and seven feet in circumference in the middle.

A hole was bored into it on the North side, five feet above the surface of the ground, eleven inches deep towards the centre of the tree, but obliquely upwards, to allow any sap, which might ooze through the wounded surface, to run out.

I then fitted to this part a box about eight inches wide and five deep, and fastened it to the tree: the bottom of the box opened like a door with a hinge. I stuffed the box with wool, excepting the middle, opposite to the hole in the tree: for this part I had a plug of wool to stuff in, which, when the door was shut, inclosed the whole. The intention of this was to keep off as much as possible all immediate external influence either of heat or cold.

The same thermometer with which I made my former experiments, seven inches and a half long, was sunk into a long feather of a peacock's tail, with a slit upon one side to show the degrees; by this means the ball of the thermometer could be introduced into the bottom of the hole.

EXP. I. March 29th, I began my experiments at six in the morning, the atmosphere at $57^{\circ}\frac{1}{2}$, the thermometer in the tree at 55° ; when it was withdrawn the quicksilver sunk to 53° , but soon rose to $57^{\circ}\frac{1}{2}$ ^(b).

This experiment was repeated three times with the same success. Here the tree was cooler than the atmosphere; when one should rather have expected to have found it warmer, since it could not be supposed to have as yet lost its former day's heat.

EXP. II. April 4th, half past five in the evening, the tree at 56° , the atmosphere at 62° ; the tree therefore still cooler than the atmosphere.

EXP. III. April 5th, wind in the North, a coldish day, six o'clock in the evening, the thermometer in the tree was at 55° , the atmosphere at 47° ; the tree warmer than the atmosphere.

(b) The sinking of the quicksilver upon being withdrawn I imputed to the evaporating of the moisture of the fluid upon the ball.

EXP.

EXP. IV. April 7th, a cold day, wind in the North, cloudy, at three o'clock in the afternoon, the thermometer in the tree was at 42° , the atmosphere at 42° also.

EXP. V. April 9th, a cold day, with snow, hail, and wind, in the North-east; at six in the evening the thermometer in the tree at 45° , the atmosphere at 39° .

Here the tree was warmer than the atmosphere, just as might have been expected. If these experiments prove any thing, it is that there is no standard; and probably these variations arose from some circumstance which had no immediate connection with the internal powers of the tree; but it may also be supposed to have arisen from a power in the tree to produce or diminish heat, as some of them were in opposition to the atmosphere.

After having endeavoured to find out the comparative heat between vegetables and the atmosphere, when the vegetables were in action; I next made my experiments upon them when they were in the passive life.

As the difference was very little when in their most active state, I could expect but very little when the powers of the plant were at rest.

From experiment upon the more imperfect classes of animals it plainly appears, that although they do not resist the effects of extreme cold till they are brought to the freezing point, they then appear to have the

power of resisting it, and of not allowing their cold to be brought much lower.

To see how far vegetables are similar to those animals in this respect, I made several experiments: I however suspected them not to be similar, because such animals will die in a cold in which vegetables do live; I therefore supposed that there is some other principle.

I did not confine these experiments to the walnut tree, but made similar ones on several trees of different kinds, as pines, yews, poplars, &c. to see what was the difference in different kinds of trees. The difference proved not to be great, not above a degree or two: however, this difference, although small, shews a principle in life, all other things being equal; for as the same experiments were made on a dead tree, which stood with its roots in the ground, similar to the living ones, they became more conclusive.

In October I began the experiments upon the walnut tree, when its powers of action were upon the decline, and when it was going into its passive life.

EXP. VI. October 18th, at half past six in the morning, the atmosphere at $51^{\circ}\frac{1}{2}$, the thermometer in the tree was at $55^{\circ}\frac{1}{2}$; but, on withdrawing and exposing it for a few minutes in the common atmosphere, it fell to $50^{\circ}\frac{1}{2}$.

EXP.

EXP. VII. October 21st, seven o'clock in the morning, the atmosphere at 41° , the tree at 47° .

EXP. VIII. October 21st, in the evening at five o'clock, the atmosphere at $51^{\circ}\frac{1}{2}$, the tree at 57° .

EXP. IX. October 22d, at seven in the morning, the atmosphere at 42° , the tree at 48° .

EXP. X. October 22d, one o'clock after noon, the atmosphere at 51° , the tree at 53° .

EXP. XI. October 23d, in the evening of a wet day, the atmosphere at 46° , the tree at 48° .

EXP. XII. October 28, a dry day, the atmosphere at 45° , the tree at 46° .

EXP. XIII. October 29th, a fine day, the atmosphere at 45° , the tree at 49° .

EXP. XIV. November 2d, wind East, the atmosphere at 43° , the tree at 43° .

EXP. XV. November 5th, wet day, the atmosphere at 43° , the tree at 45° .

EXP. XVI. Nov. 10th, atmosphere at 49° , the tree at 55° .

EXP. XVII. November 18th, atmosphere at 42° , the tree at 44° .

EXP. XVIII. November 20th, fine day, the atmosphere at 40° , the tree at 42° .

EXP.

EXP. XIX. December 2d, the atmosphere at 54° , the tree at 54° .

In all these experiments, which were made at very different times in the day, *viz.* in the morning, at noon, and in the evening, the tree was in some degree warmer than the atmosphere, excepting in one, when their temperatures were equal. For the sake of brevity I have drawn up my other experiments (which were made on different trees) into four tables, as they were made at four different degrees of heat of the atmosphere, including those made in the time of the very hard frost in the winter of $177\frac{5}{6}$. They were as follows.

1 ft.				
Atmosphere.	Names.	Height. Ft. In.	Diameter. Ft. In.	Heat. °
29 deg.	{ Carol. poplar,	2	2	$29\frac{1}{2}$
	{ Engl. poplar,	4	$2\frac{1}{4}$	$29\frac{1}{2}$
	{ Orien. plane,	3	$1\frac{1}{4}$	30
	{ Occid. plane,	3.6	2	30
	{ Carol. plane,	1	$1\frac{3}{4}$	30
	{ Birch,	3.6	$2\frac{1}{2}$	$29\frac{1}{2}$
	{ Scotch fir,	3.6	4	$28\frac{1}{2}$
	{ Cedar libanon,	2.2	$4\frac{1}{2}$	$28\frac{1}{2}$
	{ Arbutus,	2.6	$3\frac{1}{2}$	30
	{ Arbor vitæ,	2.8	$3\frac{1}{2}$	29
	{ Diffid. cyprus,	3	$2\frac{1}{2}$	30
	{ Lacker varnish,	3.6	2	30
	{ Walnut tree,	5	2.4	31

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The old hole in the walnut tree being full of sap was frozen up, but a fresh one was made.

2d.

Atmosphere.	Names.	Height. Ft. In.	Diameter. In.	Heat. °
27 deg.	{ Spruce fir,	4	2 $\frac{1}{2}$	32
	{ Scotch fir,	1.5 $\frac{1}{2}$	1 $\frac{1}{2}$	28
	{ Silver fir,	3.11	2 $\frac{1}{2}$	30
	{ Weymouth fir,	4.6	2 $\frac{1}{2}$	30
	{ Yew,	3.7	3	30
	{ Holly,	2.6	2	30
	{ Plumb tree,	4.6	3	31 $\frac{1}{2}$
	{ Dead cedar,	3.11	3	29
	{ Ground under snow,	3 deep	—	34

3d.

Atmosphere.	Names.	Heat.
24 deg.	{ Spruce fir,	23°
	{ Scotch fir,	23
	{ Silver fir,	23
	{ Weymouth fir,	23
	{ Yew,	22
	{ Holly,	23
	{ Dead cedar,	24

The same trees we mentioned when the thermometer was at 29°, in new holes made at the same height, and left some time pegged up till the heat produced by the gimlet was gone off; but in which, as they were moist

from the sap, the heat could be very little, especially as the gimlet was not in the least heated by the operation.

4th.

16 deg.	{	Car. poplar,	17°
		Eng. poplar,	17
		Ori. plane,	17
		Occ. plane,	17
		Carol. plane,	17
		Birch,	17
	{	Scotch fir,	16½

It will be necessary to observe, that the sap of the walnut tree, which flowed out in great quantity, froze at 32°. I did not try to freeze the sap of the others.

Now, since the sap of a tree, when taken out, freezes at 32°; also, since the sap of the tree, when taken out of its proper canals, freezes when the heat of the tree is at 31°; and since the heat of the tree can be so low as 17° without freezing; by what power are the juices of the tree, when in their proper canals, kept fluid in such a cold? Is it the principle of vegetation? Or is the sap inclosed in such a way as that the process of freezing cannot take place, which we find to be the case when water is confined in globular vessels? If so, its confinement must be very different from the confinement of the moisture in dead vegetables; but the circumstance of
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vegetables dying with the cold, and then freezing, appears to answer the last question. These, however, are questions which at present I shall not endeavour to solve.

I have made several experiments upon the seeds of vegetables similar to those on the eggs of animals; but, as inserting them would draw out this paper to too great a length, I will reserve them for another.

